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NATURE AND PURPOSE OF BOTANICAL CLASSIFICATION FROM THE STANDPOINT OF THE PRODUCER, PARTICULARLY WITH RESPECT TO TROPICAL PLANTS[1]

C. G. G. J. van Steenis
Rijksherbarium, Leiden, Holland

I have hesitatingly agreed to participate in this symposium, because the subject has already many times been the theme of discussion, and that, remarkably often, by theorists, like myself, who have themselves not endeavoured to compose large monographical works. My shyness, borne from lack of authority and qualification is but little compensated by the fact that I have during several decades organized and supervised classificatory work of others. For this I have deeply absorbed myself in reading the few but essential essays written on the subject by the great masters of classification; in which my preference turned to those of Hooker (1855) and Bentham (1874), which excel in clearness, conciseness, and depth.

For these reasons you cannot expect from me any novel ideas on the subject, let alone revolutionary thought, if that is anyhow feasible, but I will give some shading by emphasis on certain aspects ; from the standpoint of the producer and his incentives.

In the title of this symposium there are three key words which need comment, *purpose, nature,* and *producer.*

To start : I cannot well distinguish between nature and purpose and have blended the meaning of these two words into the concept purpose.

The nature of botanical classification is not different from that of other disciplines of science.

Its purpose is clear, viz. to provide a useful[2] and intelligent classified inventory of the plant world, living and fossil. Or, as Bentham (1874) explained : "the main object of systematic botany is not the finding out the name of a plant, but the determining its relations and affinities, the making us thoroughly acquainted with its resemblances and differences, with those properties which it possesses in common with others or which are peculiar to itself, whether these properties consist in outward form, inner structure, physical constitution, or practicable applicability to use, all of which have to be taken into account in the formation of order, genera, and their subdivisions.".

That classification results in a hierarchic array is a truism valid for all sciences, the spacing of entities and their sorting into levels being defined by degree of likeness, in short affinity. Naturally the means used in attaining the purpose have been broadened and refined in the course of time. Starting with gross morphology, which still yields the largest amount of information about structure, anatomical data (including anatomy in the strict sense, vascularization, palynology, cytology, and karyology), phytochemical data, data derived from ontogeny (embryology, blastogeny), distribution (ecology and chorology), biology (pollination and other biotic relations), and genetics (breeding systems, crossings) contribute to our final judgment. In practice most taxonomists, both in the past and the present, can only perform work on a minute or small section of this array of sources, and there is even hardly any world Herbarium equipped with specialists and apparatus to perform such a complete survey of even one group in all these aspects. For tropical trees it is even impossible to achieve.

However this may be, even with a complete research project the purpose is not fulfilled without realizing that it inevitably leads to a wider perspective of classification : the genesis of the plant world, in short evolution, and its mechanism(s). The primitiae of this philosophy are already found in the works of early classificators, Ray, Linnaeus, etc., and it has gained depth, and especially speed, after Lamarck advocated the idea of organic evolution and Darwin and Wallace tried to reveal its natural mechanism(s). The reconstruction of evolution then, is in my opinion, the ultimate purpose of classification. From this follows that, theoretically, there is only one true natural classification, as there has been only one evolutionary

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[1] Lecture held in a symposium on the *"Nature and Purpose of Botanical Classification"* during the Tenth International Botanical Congress, Edinburgh, August 1964.

[2] The term useful, taken in this context from Bentham, is of course the usual understatement, equivalent to "the best we can give". It is not to be understood as the most easy or most convenient.

The highest expression of usefulness is, therefore, not an artificial classification, though the additional framing of an artificial system may be desirable for practical reasons.

(genealogical) lineage, in Bentham's words : "affinity derived from consanguinity".

It is agreed that many taxonomists understand the purpose of their immediate task in the classification of a particular group(s) in a far more restricted sense. They are generally so much absorbed with such task(s) that they often do not correlate their work with such wide-going considerations. Still, I believe all will agree on the essence of my reasoning.

Botanical classification finds an exact parallel, both practical and theoretical, in astronomy, which classifies celestial matter and bodies, with the ultimate purpose to understand the mechanism(s) of the universe and its genesis, although many astronomers devote all their study to the properties of one particular planet, star, etc.

Then we come to the producers. There is a great variety among them, as among astronomers, depending on the aim they set themselves (scope and degree of detail they want to pursue) and collaboration and facilities (Herbarium, Library, Field work) they have, ranging from those who sort clones of pararubber, pure lines of peas, races of rice, classify grasses by the anatomy of their leaves, or cucurbits by the properties of their seeds, to monographers of genera or families, compilers of Floras local or regional, and surveys of the entire plant kingdom.

Their aim can be purely floristic, that is, naming of specimens by means of the best (most reliable) Floras or revisions and producing a list, census, at best a local Flora. But for a better understanding of a few minor taxa floristic botany seldom reaches a creative level in the way of classification ; there is for the floristic botanist no need to perform re-examination of existing classification, regionally or hierarchically. In temperate countries taxonomic production is largely floristic. There is one advantage here, namely, that the better accessibility of material facilitates critical classification of minor taxa and making use of refined methods (karyology, ecology, flower biology, experimenting, etc.) but they seldom lead to improvement of classification above the species level.

In the tropics, with their much more complicated flora, floristic botany should definitely be discouraged in the present state of knowledge. Eagerness to publish lists and enumerations should be suppressed, whatever pressure is brought forward by agriculturists and forestry services. Such publications create only more work for future creative correlative attempts and nobody nor science is served by unreliable information by premature publication. The same holds for local Floras if they are not based on regional or monographic work of a broader scope.

In passing I may remark that it is my impression that too much energy of botanists is spent on floristic work, probably partly due to the fact that many of them have never had or received an incentive towards classificatory work and never got the taste of it. I can assure them that the taste is excellent by citing the late Dr. C. A. Backer, who after decades of floristic work on the Flora of Java and at advanced age could be persuaded to do revisional work for the Flora Malesiana ; so much did he enjoy its wider scope and greater responsibility that he said : "why was this work not invented earlier?" alluding to the sad fact that he would not be able to spend many years more on these studies.

True improvement of classification can only be expected from monographical work, and in certain respects even more from the production of regional tropical Floras. Though I admit that classificatory work is nowadays, more than in the 19th century. burdened with technical tricks and extensive bibliographical work, examination of large collections, intricate nomenclatural problems, type method, etc., there is no doubt that a restricted number of qualified, devoted, tenacious private floristic botanists in temperate countries not officially attached to large herbaria could well amuse themselves with classificatory work and contribute to its frame. Brains, books, and bricks, in that sequence, are still the adage for creative scientific work.

It is often suggested that biological classification is one of personal taste or vision, especially by laymen and botanists without practical experience themselves. It is sometimes even assumed by such people and theorists that several classifications of equal merit, standing and usefulness are possible. It is held sometimes that biological classification is not comparable to that in other disciplines of the pure sciences, chemistry, physics, and astronomy, because in these disciplines classificatory data can be expressed in checkable figures, obtained by registration through objective instruments, and correlated by calculus. It should certainly be admitted that biological classification is certainly less easily and less sharply checked. Its objects are all variables which, though physico-chemical by nature, are hardly liable to experimentation beyond the species level, its definitions are more vague, and this would seem to make biological classification tend to be one of personal judgment or taste. Besides, the value of the 'characters' used for classification is not a fixed one as the same character may be significant in one group, but of much less value in another. Indeed, the methodology of comparative morphology leaves room for difference of opinion. Biological classification might seem a personal achievement, a work of personal appreciation and art rather than of science. As a matter of fact some students of mine find botanical classification far more difficult mentally than work which they had to perform on plant physiology or chemistry where they can read their instruments and follow reactions without need for forming a personal judgment on the observations which they only need to register. This is inherent in the objects and cannot be avoided.

The situation is, fortunately, less desperate than sometimes assumed. There is no hazard that muddled classifications by unqualified taxonomists and other unbalanced accounts would not be recognizable, partly by the check of the methodologies mentioned in the beginning of this lecture, but principally by the usefulness of the regional revision or monograph.

On the other hand the greater freedom of botanical classificators in framing their judgment brings along their great responsibility. Many monographs and regional Floras must serve for a long time, and proper classification is the alpha for both pure and applied botany. Classificators must therefore let usefulness prevail in presenting their work.

I have once read the opinion that if all plants and animals have been described, biological classification would have attained its ultimate goal. Apart from the fact that there are centuries of work ahead, I cannot adhere to this simplistic view. Certainly chemistry will not become superfluous, when the structure of all compounds will have been revealed, neither is crystallography 'exhausted' when all crystals and their atomic arrangements have been examined, nor genetics when all genomes have been analyzed chemically. All natural sciences will develop their scope, new questions and viewpoints will crop up, new fields will open up, and vistas extend. There is no end to our scientific curiosity.

To confine ourselves to the present immediate projects, the largest amount of classification work still to be done is that on groups in the tropical and some subtropical countries, where the Angiosperm plant world is least well known and collected, but where the largest morphological diversity is displayed. Monographers and revisors of regional Floras are here also confronted with the major frame-work classification of genera and families, and hence are forced to very critical examination of generic delimitation and even of ranks above generic level. This offers more difficulty in classificatory respect than delimitation of species, because it involves a clear insight in the classification scheme of a whole family and its morphological diversity.

The rule that classification is more liable to difference of opinion in proportion to higher rank—due to greater diversity of its constituents—we meet first in earnest with generic delimitation. Whereas diagnostic species characters are those which are common to all subspecies, varieties, or ultimately all specimens, diagnostic generic characters are those which are common to all species. If the species of a genus are not a more or less homogeneous mass of reticulate affinity, but can be assembled in clear cut groups on one or more levels, in subsections, sections, and/or subgenera, it is natural that the diagnostic generic characters are fewer than those of the lower categories. The point arises then to decide on the rank of these taxa, because any clearly defined subgeneric taxon could be raised to generic rank. There are, as far as I can see, certain guiding principles to make an acceptable judgment not impossible. In the first place the 'weight' or 'importance' of generic diagnostic characters should carry a certain degree of balance within a family. In the second place a genus should, I believe, have a certain 'structural plan' without difficulty recognizable as a concept to the observer notwithstanding the fact that most large genera possess a well-developed infrageneric differentiation at several ranks. This concept is lost if such a genus is dissolved in a multitude of micro-genera by raising each infrageneric entity to generic rank. Nothing is gained for sound classification by raising all sections of large genera such as *Ficus, Astragalus, Primula, Dendrobium, Bulbophyllum, Lysimachia,* and so forth, to generic rank. Indeed, such practice would involve the loss of the structural plan embodied by the generic concept. As no classificator could afford to deny the structural plan itself, the whole procedure of upgrading would immediately be followed by recognizing the same concept in the next higher rank, viz that of the subtribe. In the past, illustrious predecessors have gradually built up botanical classification with a generic concept which reflects a certain 'structural plan'. I believe we would do good to keep to this principle. The structural plan also reflects evolutionary development, being the backbone of the common stock ('Sippe'), with the infrageneric units as minor lines of later differentiation. It is bad enough that in certain families like the grasses and some others wholesale upgrading of subgenera, sections, and subsections has taken place to please applied botany which is said to need easy binomina. Practically, such hosts of microgenera obscure an easy insight into the frame of classification of a family ; most large genera of the *Cyperaceae* are subdivided and their subdivisions could easily be upgraded to genera. Yet no prominent cyperologist is prepared to do so. In all these cases no classificatory advantage is gained by upgrading if the relative position of the genera is maintained. Reversely, it brought, or would bring, along a nauseating disturbance in the nomenclature by which nobody is served save the fortunately few authors who let egotism and pedantry prevail over the wisdom of modesty and reticence. I fully agree with Professor Harris (1964) that such 'inflation' of taxonomy is useless and should be avoided. Only in cases where there is reasonable evidence that species or genera are arranged in a distinctly artificial or insufficiently clear classification it is clear that there is necessity for improvement of the system and we should not shun it. Such changes should, however, be based only on new facts or improved correlation or consideration. By 'artificial' we mean of course classifications which do not reflect natural affinity as far as can be obtained from a deep-going consideration in which a correlation of all available data is strived at.

Splitting of families and lower taxa onto the genus is frequently argumented as leading to *'more natural'* entitles ("mehr natuerlichen Einheiten") suggesting that it would provide a more 'natural' system. This would be only so if the more compound entity were heterogeneous in the sense of being 'artificial', that is, being composed of elements not belonging—or at least not supposed to belong—to the same lineage or genealogy ('Sippe'), so that, after splitting, the two or more parts will be placed widely apart.

A few examples may illustrate the real advantage of splitting up taxa which were 'unnatural' : Erdtman & Metcalfe (1963) showed indubitably that the genera *Kania, Thorelia,* and *Berenice* were 'unnatural', i.e. artificial in *Saxifragaceae* and really belong to *Myrtaceae, Myrtaceae,* and *Campanulaceae* respectively. Raven (1963) showed that *Boisduvalia tasmanica* is out of place in this genus and is really a (more or less deviating) species of *Epilobium.*

However, most splittings are not of this sort and 'purification' with the aim of attaining more 'natural' taxa results in keeping the split parts as close together as before the splitting, *but only in another rank.* The splitting procedure is then really a farce, based on a grave misunderstanding of the philosophy of systematy and its hierarchy. It has no scientific merit whatsoever, it brings no more useful system, it does not improve insight, and is only a testimony that the author confused the concept 'natural' with "homogeneous".

Naturally *in any hierarchical system homogeneity increases towards the lower ranks,* but this does not mean that the higher ranks are less natural than the lower ones, for example, that the tribe *Rhizophoreae* is less natural than the genus *Rhizophora,* and the latter less natural than the two series of species which can be distinguished within this genus. It is most remarkable that this confusion of the concepts 'natural' and 'homogeneous' is so frequently found in systematic argumentation by splitters. Verdcourt (1956) pointed to the absurd splitting in certain groups of animals. May botany be spared from this absurd and purposeless, completely useless chaos which lacks any sound philosophy but is based on a lamentable confusion of the concept 'natural'.

I disagree with Walters (1961) who "finds no reason to think that Angiosperm classification would be substantially the same if botany had developed in, say, New Zealand instead of Europe". Dr. Walters would be surprised to find that native tribes, in complete ignorance of scientific botany, recognize sometimes to an astonishing degree our arboreous families and genera, as is shown by the native names. They do this in the same way as we, viz. by comparative morphology.

Of course it should be admitted that tradition in descriptive botany is almost synonymous with a marked inertia, tough and resistant. Family and genus descriptions are copied from one textbook or handbook to another ; authors seem not to find any need to bring generic diagnoses up to date with newly described species or to check whether they do not contain family characters which are irrelevant in the generic description. This reluctance is borne largely from the fact that it is decidedly very time-consuming to check every species, named or unnamed, with existing traditional generic descriptions. Thus it will be perpetuated that *Dysoxylum* has spirally inserted leaves, although at least one Papuan species has them opposite, and so is the situation with respect to phyllotaxis in *Ilex, Drimys, Saurauia,* etc. Sometimes it is due to faulty understanding of the structure of a plant, as for example *Remirea* which Kunth (1837) showed to be allied to *Cyperus* more than a century ago but which everybody maintains as a separate genus in a wrong subfamily of the *Cyperaceae,* although Palla (1913) and Chermezon (1922) agreed to its affinity with *Cyperus.* Cf. Kern (1958). The reluctance is also borne from aversion of dropping well-known generic names. Everybody knows that there is only one good genus contained in the following twins or triplets : *Ranunculus-Batrachium-Ficaria-Myosurus, Gentiana-Crawfurdia, Dillenia-Wormia, Potentilla-Fragaria-Duchesnea-Comarum, Primula-Androsace,* *Centunculus-Anagallis, Aira-Deschampsia, Avena-Trisetum, Lindernia-Torenia, Veronica-Hebe, Verbascum-Celsia, Vaccinium-Agapetes, Coprosma-Nertera, Stellaria-Holosteum, Lychnis-Viscaria, Melandryum-Silene,* and several other pairs of genera in the *Caryophyllaceae,* etc., but these names are perpetuated in many works.

Another source of erroneous classification leading to tradition is the fact that families in the tropics are often revised per continent or anyway from restricted areas. Thus a genus as *Heynea,* recognized for over a century in Asia, appears not different from the American-African *Trichilia* (Bentvelzen, 1962), the Fijian *Amaroria* is nothing than a 1-seeded species of *Soulamea* (Nooteboom, 1962), the Bornean *Pseudosindora* is *Copaifera* (De Wit, 1954), the African *Afrormosia* is *Pericopsis* (Knaap-van Meeuwen, 1962), etc.

With the exception of *Remirea* no great harm is done of course in classificatory respect by over-valuation of the status of sections or subgenera by raising them to generic rank, as they will remain arranged side by side. On the other hand the same objections against this procedure are valid as for placing on one level Linnean species and microspecies, namely that taxa of the same rank become uneven in weight and that the idea of each having a structural plan is thereby abandoned.

A strict application of these two guiding lines may not rarely lead, however, distinctly into difficult situations. For example *Eleocharis* and *Scirpus* have hardly any difference in structural plan[1]. But, again, between *Scirpus* and *Cyperus* which differ only in phyllotaxis of the glumes (spiral or distichous) there are a few intermediate species. Should the three be united? Would this improve classification?

Another case : *Uncinia* differs from *Carex* only by the presence of a rachilla of which the apex is recurved, hardened, and thickened, this part being the morphological equivalent of a reduced bract. *Uncinia* has a well-defined, restricted South Pacific distributional area and is clearly a genealogical lineage ('Sippe'). The structural difference with *Carex* rests solely on this small character ; in some *Carex* species a rachilla is also developed, and this may even bear a reduced bract. However, *Kobresia* (incl. *Schoenoxiphium*) has also the same structural plan as *Carex* but has a rachilla bearing male flowers and/or an open or almost open utricle ; but some *Carex* species have also a short but distinct slit in the utricle. Clearly the three genera make part of one tissue of affinity, token of common parentage, without appreciable breaks, although it must be admitted that the number of species showing signs of transition are very small, and with *Uncinia* none[2], in proportion to the total number of species of the three entities.

In both problematic cases, *Cyperus* and *Carex* there is no magic key for improving classification, as Nature offers these plants in this way. In both cases one can defend to have two large genera each with three subgenera, or one can keep the latter apart as genera. And as Bentham expressed "it is left in a great measure to the judgment of the systematist how far he should go in dividing and subdividing, and to which of the grades of division and subdivision he shall give the names of Orders, Suborders, Tribes, Genera, Subgenera, Sections, etc." After all, the idea that genera should be of "equal" or "similar" weight is due to our human idea of orderliness, a scheme which we superpose on the facts of Nature ; it is a question whether the structure of Nature is that orderly as prescribed by the ranks of phytography.

In essential aspect generic characters are not differently obtained from specific characters, inasmuch specific characters are those common to all specimens of a species, and generic characters those common to all species of a genus, but it must be added that specific characters can be tested by experimentation and further that the number of specimens of a species is infinitely larger than that of species of even large genera enabling a wider test. From this follows that we have a firmer basis for a grip on species delimitation than for genera.

Danser (1929) advocated some decades ago the desirability to give both species and genus a genetical background. This idea was by no means new ; long ago Herbert (1837) concluded that the occurrence of hybrids is proof of their congenerity. Danser suggested to equalize species with a syngameontic population, a commiscuum, in which there is a complete miscibility[3] and to equalize the genus with a comparium, that is all species which can be crossed without or with very much reduced fertile hybrid F 2 progeny. With these concepts he tried to approach genetic conditions in nature, because indeed both comparia and commiscua are no fictions but real breeding systems in nature. Living matter is divided up into comparia, entities which are out of any genetic contamination and have obviously to proceed evolutionarily on their own, as completely isolated units.

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[1] The data on Cyperaceae I owe to Mr. J. H. Kern.

[2] There is one *Uncinia* in which the "hook" is badly developed.

[3] Danser makes an essential distinction between *crossability* and *miscibility,* the first being the ability to hybridize, the latter to have a completely fertile progeny or almost so, in which the progeny exhibits as a cocktail of characters.

The classificatory application of Danser's otherwise indisputable genetical concepts seems to meet with great difficulty, as from what is known from genetical experiments a comparium may in one case be equal to a section (for example in *Ceanothus*), in other cases to a family, like the *Gramineae* or *Orchidaceae*. These consequences seem, at least in the present state of knowledge, not acceptable, although in many cases the genus is admittedly a comparium. Another suggestion has been to define genera by an aneuploid chromosome number ; this is no more leading to practical application, though probably an appreciable number of genera are aneuploids.

What has been said about genera is largely also valid for families of which Linnaeus had already a clear idea, before Adanson and the de Jussieus. That Linnaeus did not offer his classification by way of families is due to the fact that at his time a clear and succinct circumscription of these families was impossible. And he was a man who hated vagueness. By the way, some of us still share this aversion. Linnaeus realized very well that his classes were a largely artificial survey fitting his practical aim of allowing sharp distinction and convenience for consultation. In the more than two centuries since Linnaeus, family circumscription has not been fully stabilized and it is in a flux till the present day, to judge from Hutchinson's new work (1959). This is, because with higher rank circumscription necessarily becomes more vague in proportion to increasing diversity. Still, I think we should maintain the principle that also families should have a more or less clear 'structural plan', reflecting mutual affinity, and common descent. Here, again, reticence in upgrading should be the guiding line, as it is an easy matter to raise each subfamily, tribe and subtribe to family rank. Here again no scientific gain in a classificatory sense is made by upgrading if the relative positions remain the same. For example *Siphonodon* is by some suggested to represent a family of its own, instead of a tribe or subtribe of the *Celastraceae,* but is still maintained side by side with *Celastraceae* in Celastrales. It is forgotten by such authors to study the Papuan *Brassaiantha* and the Queensland *Hedraianthera* which have also a hollow pistil and well connect *Siphonodon* with *Celastraceae.* I cannot see any profit in raising all tribes of *Loganiaceae* to family rank but maintaining them together in a new ordo *Loganiales,* as Hutchinson proposed, without providing new data for this inflation. Similarly, I can find no advantage in inflating *Magnoliaceae* sensu lato into *Magnoliales* if all families are in this upgraded classification kept together in the same mutual position, to reflect a common descent. It would have been otherwise if *Winteraceae* were split off *Magnoliales* reflecting a new vision, viz. that they were of different ancestry as *Magnoliaceae* sensu stricto and represent a distinct line of descent. Hutchinson himself says of *Winteraceae* that they are closely related to *Magnoliaceae* "though on parallel lines and more austral in distribution". If the ancestry of *Winteraceae* is indeed 'parallel' to that of *Magnoliaceae* s. str. they should not be fused into one order.

As with the genera there is a distinct tradition with the families and only few people are inclined to real re-examination of family delimitation. One is used to distinguish *Celastraceae* and *Hippocrateaceae,* but who is aware that no reasonable limits can be drawn to separate them? A similar thing can be said about *Thymelaeaceae* and *Gonystilaceae,* etc.

In the rank of the ordo we come within the Angiosperms into great practical difficulty in maintaining the principle to strive at identifying classification with the most probable genealogical descent on a sufficiently sound basis. I would prefer to have a limited number of families instead of a limited number of ordos of which the 'naturalness' is sometimes very unclear or at least very uncertain to me. The pursuit of the lower branches of the genealogical angiosperm tree becomes, by the lamentable absence of sufficient reliable fossils, a hazardous task from the standpoint of natural classification. Pulle's presentation of loose circles (1938) is probably exactly the limit which we can attain with the present state of knowledge.

Whether it will be ever possible to verify whether all our ordos can really be submitted to the testimony of common descent seems very doubtful to me, except for some of which the members are so closely allied that they could be better defined as superfamilies. There is little hope that within the next century an astounding array of fossil records will yield what we want to know. But of course nobody can tell beforehand what new methodologies may be found to pry into things now hidden to us.

With a few words on desiderata for classification of the Phanerogam plant world I will conclude this lecture. The site where creative work on the major classification of the tropical and subtropical plant kingdom, from family to species, is performed, is in a few centres in temperate parts of the world. It needs a large staff of highly trained taxonomists to perform this work, further a large collection, a large library, and, in our opinion, the urgent presence of specialists in affiliated branches of botany, at least for anatomy, for karyology, for chemotaxonomy, and for palynology, working together with the taxonomists for systematical purpose and checking ideas of gross morphology. This work should, however, emphatically be sustained by the full cooperation of institutes in the tropics and subtropics, implementing such efforts, stamping taxonomic research to a joint international affair.

The making of monographs of the medium and large families was always a huge affair, even in the concise Prodromus style of De Candolle. For the medium-sized and large families it has in our time gradually become an unwieldy task by the size of both material and bibliography. Few botanists are in a position, and besides, mentally prepared to spend a lifetime in designing a critical classification of one family only. Even the generic classification seems to offer great difficulty, to judge by the extremely slow way in which the second edition of Pflanzenfamilien is produced. And those who hurry or perform such monographs as a sideline in their spare time are apt to spoil the work. Let us not deceive ourselves: hardly any world monograph of a medium-sized to large family proves to be satisfactory in practice.

For these reasons I see more and more the great advantage of large regional Floras, and, for species, monographs of separate genera, as lines for improvement of critical classification.

Large regional Floras will, naturally, also contribute in no mean way towards classification of families and contain often monographic or submonographic genus revisions. They are very large undertakings, notwithstanding the fact that for a regional Flora it is more easy to concentrate material, bibliography, and personnel. The grade of work itself must be as critical, original and creative as that for monographs, as such Floras must serve as a firm basis or frame for later, more refined, local work. The production of such Floras is necessarily slow, partly because Hookers and Benthams are nowadays scarce. Even revised editions of existing ones seem to be beyond the capacity of contemperaneous systematists ; the few ones existing are now reprinted literatim, having up till the present maintained their immense merit.

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